

This paper serves as an introduction to the following papers, which were presented at a colloquium entitled “Earthquake Prediction: The Scientific Challenge,” organized by Leon Knopoff (Chair), Keiiti Aki, Clarence R. Allen, James R. Rice, and Lynn R. Sykes, held February 10 and 11, 1995, at the National Academy of Sciences in Irvine, CA.

Earthquake prediction: The scientific challenge

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As recently as 20 years ago the problems of earthquake prediction research were approached through a compilation of a succession of isolated case histories of presumed precursors to subsequent large and small earthquakes. The hope was that these precursory phenomena would appear before many, if not all, subsequent events. Alas, some of these hopes have either evaporated or have proved extremely difficult to document. Topics such as anomalies in the ratio of *P*- to *S*-wave velocities, magnetic fields, resistivity, tilt, emission of noble gases, and so on are no longer at the leading edge of contemporary interest. Although interest in these areas is occasionally rekindled, the spark is difficult to fan into flame, and investment of support and effort in these areas has not been heavy in recent times.

Today our approach is much the same as before: we continue to study a succession of case histories of events leading to strong earthquakes. Even today, there are occasional reports of new precursory anomalies, such as a change in the magnetic field before the Loma Prieta earthquake and, as will be discussed later in this collection of papers, observation of an increase of the concentration of chlorine and other ions in well waters before the Kobe earthquake. Whether these new areas will prove to be universals or disappear as others have remains for the future. But some avenues of phenomenology have continued to be pursued: clustering and anticlustering of earthquakes, creep measurements, changes in the attenuation factor, paleoseismicity methods, etc.

A second thread of earlier prediction research was the presumption that small earthquakes were scaled-down versions of large ones, and hence the supposition was made that the study of small earthquakes would reveal important truths about large ones, a model if not driven by the scaling implicit in the Gutenberg–Richter distribution, then at least with the notion of scaling lurking in the background. These ideas suggest that simple isolated fractures in an elastic solid have a distribution of stresses and slips that are scaled only by the sizes of the cracks and hence whatever precursors, and postcursors for that matter, that might be observed will also be similarly scaled. This direction of research has also undergone modification over the years: on present-day models, earthquake fractures take place in a prestressed solid in which fluctuations are significant perturbations of the uniformity of the stress field. But small fractures take place in the shadows cast by the stress field of the larger and the largest fractures. The chain of self-similarity is broken for the largest earthquakes, since the largest fractures do not have stress fluctuations with even larger scales to contend with. Furthermore, the details of the fracture and of the properties of nearby rocks are not resolved observationally in smaller earthquakes, certainly not as clearly as in the case of large earthquakes. Today, the paradigms have shifted to the study of strong earthquakes and away from the

more numerous small earthquakes, except insofar as the small ones give information about future large ones.

The issue of prediction has always been one of the establishment of the probability that an earthquake will occur within a specified time interval, a specified space interval, and a specified magnitude range. Contraction of these intervals remains an elusive goal. Because the techniques that are used differ, earthquake prediction research has been divided into three time intervals: those of short-term predictions which cover the time interval from a day to a few hundred days before a strong earthquake, of intermediate-term predictions covering the interval from about one year to one decade, and of long-term predictions that cover intervals longer than a decade before great earthquakes. “Earthquake prediction” in the popular language is consonant with short-term prediction. At the present time, optimism is rather low about the prospects for short-term prediction, because of its local nature: one would have to be fortunate to have instruments within short range of the future focus of a strong earthquake. Even the most promising approach, through a study of accelerated precursory creep, does not seem to be a very productive lead, since the precursor is localized in a focal zone that is at least 15 km (straight down) from the nearest instrument.

Intermediate-term prediction has significant value in the United States and other industrialized nations, because it gives a useful lead-time for the marshaling and focusing of resources for the strengthening of construction. Most efforts at intermediate-term prediction research have centered on identification of patterns of earthquake occurrence by magnitude, time, and/or location prior to strong earthquakes. In this area too, we have been obliged to rely on well-instrumented case histories of precursory clustering and anticlustering presumed to be associated with future large earthquakes. Systematization has been difficult because of the paucity of large earthquakes.

On the long time scale, the questions that are asked are whether given faults, and especially those that support the largest earthquakes, rupture periodically or not. Here the evidence is not to be gleaned from the study of seismic recordings or catalogs of earthquakes determined from the seismic recordings of the instrumental era, which starts in the case of Southern California after the Long Beach earthquake of 1933. The evidence in the long-term regime is derived from analysis of ancient faulting episodes and the interaction between the geometry of faults and the seismicity of the largest events; these are data that are difficult to obtain, because they rely largely on results of difficult geochronological measurements in excavations across faults.

Over the past one or two decades remarkable progress has been made in detailing the case histories of the precursory state before large earthquakes, including many cases in which these precursory intervals appear to be uneventful. The progress has been especially noteworthy in California where dense networks of seismographs, creep-measuring instruments, and other devices have succeeded in delineating the events precursory to the Loma Prieta (1989), Landers (1992),

and Northridge (1994) earthquakes. Progress in this field has been due to the development of a dense network of seismographs, a program that took a number of years to install. Similarly, better tools are available for the study of precursory creep and changes in the earth's magnetic and electric fields. With the new generation of instruments, and with increased resolution in devising solutions to the inverse problem, some of the older presumptions about precursors have disappeared from the repertoire as noted above, others survive with increased intensity of attack, and a few new methods appear.

We remain in the case-history stage of the study of precursory clustering of earthquakes prior to strong earthquakes; these accounts of clustering remain without statistical substantiation because of the paucity of strong earthquakes for study. These comments should not be interpreted to be a plea that more strong earthquakes should take place.

Great progress has been made on the modeling front, partly through the extraordinary development of large-scale computing resources. There has been a remarkable increase in our understanding of the behavior of the deformation of rocks through laboratory measurements, and especially in the behavior of prefractured rocks, in the times before large-scale rupture. There has been unusual activity in the modeling, usually numerical, of processes of self-organization of the stress field due to the occurrence of extended fractures in faulted systems. In particular, we have acquired insights into the physics of fracture, on preexisting, nonuniform faults.

Despite the optimistic tone of the above remarks, an ability to predict earthquakes either on an individual basis or on a

statistical basis remains remote. It is clear that the scientific issues must be understood before routine predictions can be announced, which in a generalized sense is an engineering problem.

There are other issues connected with earthquake prediction that were not discussed at the colloquium presented here: neither the organization of national programs in earthquake prediction, nor the engineering problems, nor the problems of societal response to possible future predictions in the three different time scales. With regard to the scientific issues, the colloquium committee developed a program that focused in roughly equal amounts on the laboratory and modeling research that is currently being performed on the one hand and on the observations relevant to the three time scales on the other. The papers that follow are an excellent representation of the thoughts that were aired and cover the full range from the pessimistic to the optimistic.

It is a certainty that the problems of societal response and engineering response to earthquake predictions are not going to be solved until the scientific problems can be brought under control. These are no more difficult than they were several decades ago; they are only more clearly defined today. We recognize today that the scientific problems are not simple.

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